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- Fig. 5. Three normal segments, dorsal view, less magnified than fig. 3.
- Fig. 6. Diagrammatic cross-section, showing relative positions of the parts of the exo-skeleton.
- Fig. 7. Last five segments, dorsal view.
- Fig. 8. Same, ventral view.
- Fig. 9. Antenna, much magnified, ventral view.
- Fig. 10. Second leg of male.
- Fig. 11. Eleventh leg of male.
- Fig. 12. Last leg of male.
- Fig. 13. Male copulatory legs, dorsal view.
- Fig. 14. Same, ventral view.
- Fig. 15. Segments seven and eight of young male.—O. F. Cook.
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## PSYCHOLOGY.<sup>1</sup>

**Congress of Psychologists.**—The third Congress of Psychologists was held at Munich, August 4th to 9th, Prof. Stumpf, of Berlin, presiding. It was the largest and in many respects the most successful of the three. Of course the German attendance was fuller than at the last one, held in London in 1892, and German delegates are always most welcome. When we take into account the fact that Germany is to-day the country where psychology is most vigorously and successfully pursued, it follows that this Congress was, up to date, the greatest gathering of eminent psychologists ever seen. As to France, the attendance was disappointing in numbers, although the delegation was very representative; and the same is true of the British contingent. The other countries, except America, were adequately represented; the small attendance from our side of the water being a matter of the more surprise in view of the tendency of our professors to take their vacations abroad—indeed, the attendance at the last Congress in London was considerably larger.

In its general character, the tendency to allow the popular attendance upon the meetings to swamp the scientific proceedings was more marked in Munich, and it is not too much to say that this constituted a very great defect in the arrangements. The membership was over four hundred. There was a constant flow from hall to hall, and the corridors

<sup>1</sup> Edited by H. C. Warren, Princeton University, Princeton, N. J.

were filled with bewildered persons. Some limit must be put on the popular membership at the next congress, or the scientific people will yield the field to the sightseers and amateurs. The other possible improvement comes to the front again apropos of this meeting in Munich—the crowded condition of the programme. Besides the general meetings, which came in the forenoon sessions, the committee arranged for five sections, all running simultaneously and all subject to constant give and take, as respects their audiences, from one to another. Besides the constant interruptions and great confusion which this produced, it practically prevented a person from hearing many readers whom he especially desired to hear. Since the time limits were not enforced upon the papers or discussions, one could never tell how far on this section or that had progressed, and so could not time his presence for any particular reader. Moreover, the papers were as usual so generally accepted by the committee—anyone who wanted to present something had only to send his name and topic beforehand—that many were read which were of little or no scientific value; and the titles of papers were entered on the programme in advance, so that there was no way to learn infallibly whether a particular reader had arrived and would present his dissertation or not. The gaps left by the absentees were consequently quite an unknown quantity. Every such meeting should have a committee to read and select from available papers, arrange them strictly according to unchangeable time divisions, and require each reader to report finally a day or two before the meeting as to his actual attendance, the final programme being only then printed. This would have the further advantage of ruling out titles and names which are from the first doubtful; for it is astonishing to what an extent men fail to carry out what should be their serious intention when they give their names to be printed on these Congress programmes.

So much for the general character of the Congress. Of course, this is not the place for an account, in any detail, of its scientific features. The division into sections will show something of the remarkable range that modern psychology finds itself obliged to take: “Normal,” “Sleep and Hypnotism,” “Mental Pathology,” “Neurology,” “The Senses and Psychophysics”—the titles being somewhat abbreviated in this list. In each of the sections there were some great papers and one or more lively discussions. The most interesting thing in the way of neurological work—it was presented, however, in one of the general meetings—was the paper by the veteran Flechsig on “Association Centres.” It will be remembered that Prof. Flechsig has been engaged for some time on comparative studies of the brains of human infants at different ages,

attempting so to arrive at a view of the order of development of the elementary mental functions, with the corresponding progress in brain anatomy and physiology. He has published very rich results from time to time, and among them is his determination of certain so-called "association centres." He thinks that the much discussed frontal region of the brain is the location of associations of a higher and more abstract kind; and that in the region back of the well-known "motor region," extending to the visual centre in the occipital region, is a great centre for the associations which bind the sense functions together. This in brief, and without the discriminations which an accurate account of his views should make. The reason which he gives for these determinations is that only after some growth, and after the senses are well developed, do we find the great masses of connecting fibres which traverse these regions forming in the child's brain.

Apart from the question of fact, as to which Prof. Flechsig's researches may be considered as being of the greatest importance (especially when we consider his method), it is difficult to see how these regions can be, in any true sense, "association centres;" for, admitting that the connections between the sense centres run through these regions, the main thing about the associations must be the things associated, not the mere fact of connections between them. One would hardly call the bunch of telegraph wires on the housetop a "communication centre;" the loci of communication are still at the telegraph offices. Without them, the wires would be possibly even more helpless than the offices without the wires. Prof. Flechsig's paper was a model for imitation in the manner of its presentation, and its interest was enhanced by slides showing the infant's brain, in sections illustrating the periods of its growth.

Another contribution to the understanding of the relation of psychology and brain physiology was that of the well known neurologist, Prof. Edinger, of Frankfort, on the question, "Can Psychology make use of the results so far attained in Brain Anatomy?" He did not confine himself to anatomy, but presented a series of interesting notices on the development of the nervous system in the scale of life, and made a strong plea for a corresponding genetic study of comparative psychology. Genetic psychology, he says, is so far behind analytic psychology because psychologists have confined their attention, on the anatomical side, to the cerebral hemispheres, while what they should do is to study the evolution of the nervous system all the way up, and see the progress of consciousness with it. "*Gerade auf diesem Gebiete müssen anatomisch-physiologische und psychologische Studien durchaus Hand in Hand gehen.*" All this is true and remarkably opportune, I think,

despite the fact, that in his main illustration Prof. Edinger falls into one of the glaring fallacies into which this sort of analogy between body and mind may lead. He says there are certain creatures (fishes) which have no hemispheres, and it follows that, on the psychological side, we must deny to these creatures "all that the hemispheres are necessary for in the higher creatures." This overlooks the great principle that, in the lower forms, less differentiated structures may do what more differentiated ones do in the higher forms. To press this point consistently, he would seem to have to deny consciousness altogether to these fishes. The lesson of this paper, however, is a most timely one; psychologists, especially in Germany, are not half awake to the genetic problem, and when they do awake, no doubt it is true that the richest lessons that the physiology of the nervous system will have to teach them will be derived from such comparative study as Prof. Edinger advises.

Several papers of general interest were read in the open meetings. The President's address was rather more severe and *wissenschaftlich* than the earlier addresses of the presiding officers have been, but it was an exceedingly interesting and discriminating review of theories on the connection of mind and body. The arraignment of Parallelism was very effective—possibly more so than the positive doctrine of the paper. Prof. Ebbinghaus of Breslau gave a new way of testing the mental condition of school children at different periods and in different conditions of fatigue, etc. It differs from the methods already in vogue in that it endeavors to test the child's correlating or apperceptive faculty rather than his sense-perceptions or his memory. The method, which teachers will find extremely interesting, consists in taking a passage from some interesting narrative-text, and, after striking out various words and phrases and printing the passage with black spaces where these erasures have been made, telling the child to fill in the spaces as he thinks the sense requires. This requirement certainly calls upon the child for more than memory, and the results of its application, as reported by Prof. Ebbinghaus, seem to show its superiority; but it would appear to be applicable to children of a more advanced age, after the memory tests are outgrown. This general judgment, however, I must make with reservation, since the synopsis of the paper did not reach my hands. This may suffice to indicate the scope of the method, and to call the attention of our educational authorities to it. They will also be interested in Prof. Ebbinghaus's severe criticism of what he called the "American method" of testing the mental condition of school children by the memory tests.

The fact that the papers on "Hypnotism" were fewer than in earlier congresses, in proportion to the entire number, and that there were a bare half-dozen on thought-transference and telepathy, shows the general tendency of psychology. The hypnotic period is past, even in France. Not that the gain from the study of hypnotism has not been permanent and great; on the contrary, its results are only now getting so absorbed into the body of psychological truth that it no longer makes sensational appeals for a hearing. As to telepathy, I think there is a real decay of interest in the subject, much as this is to be deplored. The most interesting paper in the hypnotic field was a general one by Prof. Pierre Janet.

The section on the Senses and Psychophysics did much exact work. Dr. Stratton of the University of California communicated some valuable experiments of his own on the artificial reinverting of the retinal image and its effects on the sense of bodily position in space, which will be of especial interest to those who think the normal inversion of the image requires a theory.

Two other general questions of great interest were discussed, with as much ability as vehemence, by the Vice-President of the Congress, Prof. Lipps, of Munich. One of his papers was a very important contribution in the sadly neglected field of the æsthetics of visual form. I can do no more than recommend his paper in the Congress "Proceedings" (to appear very soon) to those who are concerned with elementary æsthetic principles. The other topic was the much-discussed one on the "Unconscious" in psychology. The question, Can mental states be unconscious? has a peculiar fascination, because of the great number of verbal distinctions of which it admits. It must be confessed that Prof. Lipps's paper did not make the number of these verbal distinctions less. He reaches a sort of return to the soul-substance theory—a hidden thing in which mental states, and especially tendencies of an active kind, may be preserved when we are not conscious of them. This has long ago been refuted as a general conception, I think; but the main point of interest, and that for which I bring the matter up, is that the results of pathology, dual consciousness, "multiple personality," etc., which are considered by many as giving the strongest evidence for the "unconscious," require quite a different theory. The "unconscious" of the pathologists is a body of conscious data gathered into a new and secondary consciousness of its own. While these states of mind are not conscious to the major person—and so, by a certain license, are called "unconscious"—still it is just the evidence that they are conscious in their own way and in their own seat in the nervous

system that enables us to say that they are mental. So all this evidence goes, after all, to show a correspondence between the mental and the conscious. This Prof. Lipps does not seem to see, and his treatment of the question from a purely verbal and analytic point of view was consequently very inadequate.

In the higher fields of ethics and anthropology there were interesting papers, of which my space allows the mention of only one on "Ethical Values," by Prof. Ehrenfels (just called from Vienna to Prague), and one on the "Category of Individuality in Savage Thought," by Mr. Stout, the editor of *Mind*. Mr. Stout, I may add, has just been called to a lectureship in comparative psychology in the University of Aberdeen—a novelty for the British Isles, but appropriate in the institution which Prof. Bain has made famous in connection with psychological study. The next Congress is to meet in Paris in 1900 in connection with the Universal Exposition. Prof. Ribot will be President; M. Ch. Richet, Vice-President, and M. Pierre Janet, Secretary. The International Committee for the Paris meeting has the following American representatives: Profs. James (Harvard), Titchener (Cornell), Hall (Clark), and Baldwin (Princeton).

I cannot close this letter without referring—with profound regret, which many other American students of philosophy must also feel—to the death Prof. Avenarius, of Zurich, on August 18. Where I now write, the feeling that one of the greatest philosophical thinkers of Europe no longer adorns a Swiss university is very acute; and those who know the work of Prof. Avenarius must feel it also, regardless of the place of their habitation.—J. MARK BALDWIN, in *New York Evening Post*, Sept. 12.

**Mental Action During Sleep, or Sub-Conscious Reasoning.**—Shortly after reading the interesting article by Professor Cope with regard to recent ethnological discoveries in Assyria, undertaken under the auspices of the University of Pennsylvania, and elucidated by Professor Hilprecht, I met with the account of a peculiarly curious dream which had been experienced by Professor Hilprecht whilst his mind was deeply occupied with these very investigations.

It is of course well known to all students of mental psychology, that the most complicated, abstruse forms of reasoning have often been carried out in dreams; and many interesting and well authenticated cases of this phenomenon will be found in Dr. Carpenter's *Mental Physiology*.<sup>2</sup> But the peculiarity of Dr. Hilprecht's dream consists in

<sup>2</sup> Chap. XIII, Unconscious Cerebration. *Mental Physiology*. W. R. Carpenter, M. D. Chap. XV, Of Sleep, Dreaming and Sonnambulism, pp. 534, 593-5.

the intensely dramatic manner in which the solution of the problem he was engaged on was conveyed to his mind. I will now simply quote from the account given to Prof. William Romaine Newbold, by Professor Hilprecht, in the first place of a train of sub-conscious reasoning during sleep which put him on the track of a satisfactory rendering of an Assyrian proper name; and in the second place of the work carried out under the influence of a strangely dramatic dream.<sup>3</sup>

"During the winter 1882-3, Professor Hilprecht was working with Professor Friedrich Delitzsch, and was preparing to publish as his dissertation, a text, transliteration and translation of a stone of Nebuchadnezzar I, with notes. He accepted at that time the explanation given by Professor Delitzsch of the name Nebuchadnezzar, 'Nabû-kudurru-usur,' 'Nebo protect my mason's pad' or mortar board,' *i. e.*, 'my work as a builder.' One night, after working late, he went to bed about two o'clock in the morning. After a somewhat restless sleep, he awoke with his mind full of the thought that the name should be translated 'Nebo protect my boundary.' He had a dim consciousness of having been working at his table in a dream, but could never recall the details of the process by which he arrived at this conclusion. Reflecting upon it when awake, however, he at once saw that *kudurru*, 'boundary,' could be derived from the verb *kadaru*, to enclose. Shortly afterwards he published this translation in his dissertation, and it has since been universally adopted."

Mr. Newbold then gives a translation from the account written in German by Prof. Hilprecht of his remarkable dream.

"One Saturday evening, about the middle of March, 1893, I had been wearying myself, as I had done so often in the weeks preceding, in the vain attempt to decipher two small fragments of agate which were supposed to belong to the finger rings of some Babylonian. The labor was much increased by the fact that the fragments presented remnants only of characters and lines, that dozens of similar small fragments had been found in the temple of Bel, at Nippur, with which nothing could be done, that in this case furthermore I had never had the originals before me, but only a hasty sketch made by one of the members of the expedition sent by the University of Pennsylvania to Babylonia. I could not say more than that the fragments, taking into consideration the place where they were found and the peculiar characteristics of the cuneiform characters preserved upon them, sprang from the Cassite period of Babylonian history (ca. 1700-1140 B. C.); moreover, as the first character of the third line of the first fragment seemed to be KU, I

<sup>3</sup> Proceedings of the Society for Psychical Research, for June, 1896, pp. 13-17.



ascribed this fragment, with an interrogation point, to King Kurigalzu, whilst I placed the other fragment as unclassifiable, with other Cassite fragments, upon a page of my book where I published the unclassifiable fragments. The proofs already lay before me, but I was far from satisfied. The whole problem passed again through my mind that March evening before I placed my mark of approval under the last correction in the book. About midnight, weary and exhausted, I went to bed, and was soon in deep sleep. Then I dreamed the following remarkable dream. A tall, thin priest of the old pre-Christian Nippur, about forty years of age, and clad in a simple abba, led me to the treasure chamber of the temple on its southeast side. He went with me into a small, low-ceiled room without windows, in which there was a large wooden chest, while scraps of agate and lapis lazuli lay scattered on the floor. Here he addressed me as follows: 'The two fragments which you have published separately upon pages 22 and 26, belong together, are not finger rings, and their history is as follows: King Kurigalzu (ca. 1300 B. C.) once sent to the temple of Bel, among other articles of agate and lapis lazuli, an inscribed votive cylinder of agate. Then we priests suddenly received the command to make for the statue of the god Ninib a pair of earrings of agate. We were in great dismay, as there was no agate as raw material at hand. In order to execute the command there was nothing for us to do but cut the votive cylinder into three parts, thus making three rings, each of which contained a proportion of the original inscription. The first two rings served as earrings for the statue of the god; the two fragments, which have given you so much trouble, are portions of them. If you will put the two together you will have a confirmation of my words. But the third ring you have not yet found in the course of your excavations, and you never will find it.' With this the priest disappeared. I awoke at once, and immediately told my wife the dream, that I might not forget it. Next morning—Sunday—I examined the fragments once more in the light of these disclosures, and to my astonishment found all the details of the dream precisely verified in so far as the means of verification were in my hands. The original inscription on the votive cylinder read: 'To the god Ninib, son of Bel, his lord, has Kurigalzu, pontifex of Bel, presented this.'

"The problem was thus at last solved. I stated in the preface that I had unfortunately discovered too late that the two fragments belonged together; made the corresponding changes in the 'Table of Contents,' pp. 50 and 52; and it being not possible to transpose the fragments, as the plates were already made, I put in each plate a brief reference to the other. [Cf. Hilprecht, "The Babylonian Expedition of the Uni-

versity of Pennsylvania," Series A, Cuneiform Texts, Vol I, Part 1, "Old Babylonian Inscriptions, chiefly from Nippur.] H. V. Hilprecht."

Upon the priest's statement that the fragments were those of a votive cylinder, Professor Hilprecht makes the following comment:

"There are not many of these votive cylinders. I had seen, all told, up to that evening, not more than two. They very much resemble the so-called seal cylinders, but usually have no pictorial representation on them, and the inscription is not reversed, not being intended for use in sealing, but is written as it is read."

Then there follows a transliteration of the inscription in the Sumerian language. Mrs. Hilprecht's statement is as follows:

"I was awakened from sleep by a sigh, immediately thereafter heard a spring from the bed, and at the same moment saw Professor Hilprecht hurrying into his study. Thence came the cry, 'It is so, it is so!' Grasping the situation, I followed him and satisfied myself in the midnight hour as to the outcome of his most interesting dream."

Signed, "J. C. Hilprecht."

A few weeks after the occurrence of this curious dream, there appeared a difficulty which Professor Hilprecht was not able to explain. "According to the memoranda in our possession, the fragments were of different colors, and, therefore, could have scarcely belonged to the same object. The original fragments were in Constantinople, and it was with no little interest that I [Mr. Newbold] awaited Prof. Hilprecht's return from the trip which he made thither in the summer of 1893. I translate again his own account of what he then ascertained.

"In August, 1893, I was sent by the Committee on the Babylonian Expedition to Constantinople, to catalogue and study the objects got from Nippur, and preserved there in the Imperial Museum. It was to me a matter of the greatest interest to see for myself the objects which, according to my dream, belonged together, in order to satisfy myself that they had both originally been parts of the same votive cylinder. Halil Bey, the director of the museum, to whom I told my dream, and of whom I asked permission to see the objects, was so interested in the matter that he at once opened all the cases of the Babylonian section, and requested me to search. Father Scheil, an Assyriologist from Paris, who had examined and arranged the articles excavated by us, before me, had not recognized the fact that these fragments belonged together, and consequently I found one fragment in one case and the other in a case far away from it. As soon as I found the fragments and put them together, the truth of the dream was demonstrated *ad oculos*—they had, in fact, once belonged to one and the same votive cylinder.

As it had been originally of finely veined agate, the stone-cutter's saw had accidentally divided the object in such a way that the whitish vein of the stone appeared only upon the one fragment, and the larger gray surface upon the other. Thus I was able to explain Dr. Peters' discordant descriptions of the two fragments."

There are, says Mr. Newbold, two especial points of interest in this case, the character of the information conveyed, and the dramatic form in which it was put. The apparently novel points of information given were:

1. That the fragments belonged together.
2. That they were fragments of a votive cylinder.
3. That the cylinder was presented by King Kurigalzu.
4. That it was dedicated to Ninib.
5. That it had been made into a pair of earrings.
6. That the "treasure chamber" was located on the southeast side of the temple.

We have a *point de repère* for the treasure chamber part of the dream, in the fact, that Dr. Peters, as far back as 1891, had told Professor Hilprecht of the discovery of a room in which were remnants of a wooden box, while the floor was strewn with fragments of agate and lapis lazuli.<sup>4</sup> The other points in the dream may be accounted for by the direction in which Professor Hilprecht's thoughts had been travelling, or they may not; I must confess to thinking they cannot all be so accounted for.—ALICE BODINGTON.

NOTE.—I would advise anyone interested in the subject of subconscious reasoning in dreams, to read at length the account given by William A. Lamberton, Professor of Greek in the University of Pennsylvania, of a dream in which he solved *geometrically* a difficult problem which he had attacked from its algebraic and analytic side. The *point de repère* here seems to have been a blackboard which had formerly had a functional use in the room, but which had been painted over, the black still showing through the white paint. Professor Lamberton, on opening his eyes one morning, about a week after he had determined to banish this insolvable problem from his mind, saw upon this blackboard surface a complete figure, containing not only the lines given by the problem, but also a number of auxiliary lines, and just such lines as without further thought solved the problem at once.

"I sprang from bed," says Prof. Lamberton, "and drew the figure on paper; needless to say, perhaps, that the geometrical solution being

<sup>4</sup>Two curious cases of the dramatic form taken occasionally by dreams will be found on p. 18, Proceedings S. P. R. for June, 1896.

thus given, only a few minutes were needed to get the analytical one." (Sub-Conscious Reasoning, Proc. S. P. R. pp. 11-13).—A. B.

**"The Mimetic Origin and Development of Bird-language,"** and **"The Evolution of Bird-song."**—When one considers how many people are thinking at the same time, it does not seem strange that two persons, though widely separated and totally unknown to each other, should sometimes think not only of the same subjects but also follow in the same direction and practically at the same time, the same lines of original thought and investigation. Some of these duplicated ideas are of value in commerce; others are mere metaphysical speculations, possibly suggested by the same incidents; but at this late stage in the knowledge of natural history it does appear unusual that two people in different hemispheres and observing totally different species of animals should have simultaneously pursued independently the same far-reaching but novel line of speculative thought.

A few days ago, Mr. J. E. Harting called my attention to an article in THE AMERICAN NATURALIST, entitled "The Mimetic Origin and Development of Bird-language," which appeared in that journal for March, 1889. He did so because I had lately written a book on the subject, *The Evolution of Bird-song* (London, A. & C. Black, May, 1896), and because the article in question discussed some of my themes. I have just finished a perusal of the article, which was indeed rather exciting, since in nearly every paragraph I found an anticipation of some theory or observation of my own, which I had theretofore believed to be original. In fact, any one reading the article and afterwards reading *The Evolution of Bird-song*, would think that I had borrowed without acknowledgment a good many ideas thrown out by Mr. Samuel N. Rhoads, the writer of the essay in question. However, I am able to prove that in 1888 I had already made investigations on exactly the same lines as Mr. Rhoads, and had recorded the results of them in writing. In the summer of 1887 I began to make systematic records of the imitations I heard from imitative wild British birds, and in the course of this study various themes were attacked, such as "the influence of combat," "the influence of the love-call," "family-voices," and "the influence of imitation," etc. I wrote essays on these themes and sent them to the late Professor Harker, F. L. S. and some of them to Mr. S. S. Buckman (now an eminent geologist), with whom I had many conversations on the subjects in question. In 1890 these observations appeared, in a highly condensed form, in *The Zoologist*, in two papers entitled "The evolution of bird-song," and published respectively in

July and August of that year. I had then of course no idea that my seemingly daring suggestions that mimicry had attuned the cries of birds to their environment, had been confirmed or anticipated in America by a writer who, whatever may be the value of his deductions, is obviously an acute observer. My papers in *The Zoologist* were severely handled last year by a writer who certainly had never heard of Mr. Rhoads' article; and this year the same writer, in favorably criticising in *Nature* my new book, employed a few congratulatory words upon my having allowed certain of my former conclusions to drop into the background.

Although Mr. Rhoads and I were working on the same track, he will I am sure allow that I have gone into the subjects in much greater detail than would be possible within the limits of an article, unless it occupied the whole of the magazine in which it appeared. Mr. Rhoads traces the origin of certain tones to noises produced by the elements, such as the bubbling of air through mud, the murmurs of streams, the sibilant sounds caused by branches being rubbed against each other by the wind, the cries of the victims of predacious birds, the croakings of amphibians, and the moaning of wind in hollow trees; and I adopted the same position, though quoting different instances, in relation to each of these features. He touches on heredity, in a way that would suggest the working-out of the theme in much the same way as I have attempted to do it. In his suggestion of the original use of the voice in "hissing or choking sounds," and in my surmise that the voice was "evolved from a toneless puffing indicative of anger, or from snorts or grunts accidentally caused," in support of which idea some pertinent evidence may be adduced, we have both, I think, advanced somewhat from Darwin's proposition that involuntary and purposeless contractions of the muscles of the chest and glottis, due to excitement of the sensorium, first gave rise to the emission of vocal sounds (*Expression of the Emotions*, pp. 83, 84).

In order to show, however, that I have not been limited to only those themes which Mr. Rhoads treats in so terse and yet so attractive a style, I would mention merely the "contents" of one of my chapters, in which some side-issues are dealt with, as follows:

"Songs are generally uttered by males: exceptions—Not until birds have attained full size: exceptions—Most frequently at morning and evening: influence of weather—Tendency to rise in pitch with vehemence—Only small birds properly sing—Singers arboreal birds generally—Effect of living amid foliage: on size, hearing, and voice—Accent in songs—Singers clad in sober hues—Devel-

opment of the eyes in detecting danger—Necessity of leisure—Labours of parent-birds—Laborious and stealthy birds habitually poor in song—Flight in song: for purposes of display—Fluttering of wings a means of address—Ventriloquism—Singing in chorus.”

The study of bird-song, on the lines indicated by Mr. Rhoads and myself, is so new and so delightful a pursuit that I hasten to ask readers of *THE AMERICAN NATURALIST* to peruse his paper once more; for it is in America, so richly blessed with birds, that such investigations can be most easily developed, and that they promise the most accurate and helpful results.—CHARLES A. WITCHELL.

**A Note on Dr. Herbert Nichols' Paper** (*Amer. Nat.*, Sept., 1896).—Readers of Dr. Herbert Nichols' paper on my article entitled “A New Factor in Evolution” will understand that its intemperate spirit should rule out all reply. I may say, however, that Dr. Nichols' “home thrusts” are all directed at my view of pleasure and pain, which he considers, quite mistakenly, the point of my paper. On the contrary the “factor” is entirely the influence of the individual's adaptations on the course of evolution; not at all the particular way in which the individual makes its adaptations. I took pains to reiterate this distinction in the paper, saying (*AMER. NAT.*, 1896, p. 542-3) “So far we have been dealing exclusively with facts . . . without prejudicing the statement of fact at all, we may inquire into the actual working of the organism in making its adaptations. . . . Before taking this up, I must repeat with emphasis that the position taken in the foregoing pages, which simply makes the fact of ontogenetic adaptation a factor in (race) development, is not involved in the solution of the farther question as to how the adaptations are secured.” So I see absolutely no point in Dr. Nichols' criticisms.

The other question, which involves pleasure and pain, is discussed in the latter part of my paper; but it is not that, but my book which Dr. Nichols attacks with the grossest misunderstanding. I do not at all believe the main things which he attributes to me; first, the position that there are no pain nerves, and second, that there is a “psychic factor” which is an “efficient cause” in evolution. Psychologists know Dr. Nichols' hobby and allow for his intemperateness.

J. MARK BALDWIN.

Princeton, Sept. 25, 1896.